

When the Press Falls Silent: The Impact of Local Newspaper Closures on Corporate Greenwashing

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Abstract

This study examines whether local newspaper closures increase corporate greenwashing. Using newspaper shutdowns as an exogenous shock to local information availability and a panel of U.S. firms from 2002 to 2017, we find that firms located in affected counties engage in significantly more greenwashing. The effect is robust across alternative treatment definitions, model specifications with various fixed effects, different greenwashing measures, and additional county-level controls, indicating that the loss of local news coverage weakens external scrutiny and allows a larger gap to emerge between environmental disclosure and actual performance. Cross-sectional analyses show that the effect is stronger among larger and more visible firms, in communities with weaker community-based scrutiny, in counties with limited internet-based information substitutes, and among firms with stronger reporting incentives, consistent with local newspapers serving as an important complementary external monitoring channel. Finally, the analysis of capital market consequences shows that these disclosure responses have valuation implications, with firms exhibiting higher pre-treatment greenwashing experiencing relatively more favorable valuation effects following newspaper closures. Overall, the findings highlight the role of local newspapers in promoting environmental transparency and constraining corporate greenwashing.

Keywords: Greenwashing, Local Newspaper Closures, Information Environment, External Scrutiny

JEL Codes: G38, M14, Q56, L82

1 Introduction

Corporate greenwashing, defined as the strategic misrepresentation of environmental performance, has become an increasingly pressing concern as firms respond to growing investor and stakeholder pressure related to environmental, social, and governance (ESG) standards. This study examines how the surrounding information environment, with a particular focus on local media scrutiny, influences corporate incentives to engage in greenwashing. Given that ESG disclosures are largely voluntary and unaudited, many firms are tempted to selectively report positive environmental outcomes while concealing negative ones, thereby creating misleading impressions of sustainability (Delmas and Burbano, 2011; Lyon and Maxwell, 2011). These distortions are further intensified by weak regulatory enforcement and inconsistent global standards (Marquis et al., 2016; Heese et al., 2022), leaving stakeholders dependent on corporate self-disclosures with limited assurance of their accuracy.

Firms may strategically use ESG disclosure as a symbolic tool to manage external perceptions and maintain legitimacy. Firms face pressure to align their public disclosures with prevailing social expectations, in part because corporate image has economic value and reputational capital can lower firms' cost of capital or improve operating outcomes (Dowling, 1993). As a result, firms have strong incentives to communicate environmental responsibility even when underlying practices do not fully align with such claims.

Related research emphasizes that sustainability disclosure is often used instrumentally to shape stakeholder perceptions rather than to reflect substantive environmental change (Cho and Patten, 2007; Ntim and Soobaroyen, 2013). In this view, ESG disclosure provides a convenient way for firms to signal environmental commitment to external audiences at relatively low cost. Consistent with this argument, prior studies document that ESG statements frequently reflect symbolic efforts rather than real operational adjustments (Cho et al., 2012; Romero et al., 2019). Moreover, greenwashing may generate tangible financial benefits by enhancing firms' perceived environmental image, particularly when stakeholders view the firm as environmentally responsible (Spack et al., 2012; Parguel et al., 2015; Gillan et al.,

2015).

Building on these insights, recent work highlights that firms often operate under conflicting economic and societal pressures that make it difficult to fully reconcile sustainability commitments with core business objectives (Cho et al., 2015). In response, firms may separate external sustainability communication from internal practices by constructing symbolic structures that satisfy stakeholder expectations while core operations remain unchanged. Importantly, this form of symbolic disclosure is more likely to persist when external monitoring weakens, as discrepancies between environmental claims and actual behavior are less likely to be challenged or exposed. This perspective helps explain why greenwashing can persist and intensify following a decline in local journalistic oversight.

The decline of local newspapers presents a compelling setting to evaluate how external information shocks affect corporate environmental actions and communications. While firms may respond to stakeholder pressure through strategic ESG disclosure, the presence of local journalistic oversight acts as a crucial accountability mechanism. Local newspapers play a critical role by sharing corporate disclosures with broader audiences, enhancing the visibility of ESG efforts, and strengthening the incentives for environmental reporting (Dyck et al., 2010; Heese et al., 2022).

Therefore, the steady wave of newspaper closures across U.S. counties has created a quasi-natural experiment for studying corporate behavior. This setting offers a valuable degree of exogeneity, as closures are often driven by industry-wide technological or economic changes, rather than firm-specific factors. Jiang and Kong (2023) argue that robust information ecosystems, such as local newspapers, are essential in holding companies accountable for their environmental disclosures. Their study demonstrates that the closure of local newspapers, which serve as an important channel for monitoring corporate and environmental practices, diminishes public scrutiny and reduces corporate accountability. This finding aligns with Dyck et al. (2010), who show that media coverage can deter corporate misconduct by exposing unethical practices, and with Campa (2018), who highlights the role of

consistent media presence in promoting better corporate governance. These studies suggest that the absence of adequate information dissemination channels can increase the likelihood of opportunistic corporate behaviors, such as greenwashing. Building on these insights, this study leverages the exogenous timing of newspaper closures to examine how diminished local media oversight affects firms’ incentives to engage in greenwashing. By focusing on a quasi-natural experiment in the information environment, the analysis offers new causal evidence on the relationship between media scrutiny and corporate environmental disclosure.

Despite increasing academic attention to greenwashing, important gaps remain in the literature. While studies such as Li et al. (2022) and Torelli et al. (2020) have examined the drivers and consequences of greenwashing—ranging from its relationship with financial performance to its role in stakeholder communication—few works have directly investigated how external information channels shape firms’ greenwashing behavior. Although prior research has considered multiple factors influencing greenwashing, less is known about how external scrutiny—especially from local news—interacts with firms’ disclosure strategies. The complexity of selective reporting remains underexplored, particularly in self-regulated ESG settings where firms face weak enforcement but strong reputational pressures. This gap highlights the need for deeper investigation into how firms respond to changes in their local information environment, and how such changes shape incentives for symbolic versus substantive environmental disclosure.

Local newspapers have historically served as important monitors of corporate conduct, especially in localized settings. As Miller (2006) and Dyck et al. (2008, 2010) observe, the local press plays a unique role in holding nearby firms accountable by uncovering and publicizing misconduct, including symbolic environmental disclosures. Their close ties to the community—including access to employees, suppliers, and regional networks—enable them to detect and report practices that may escape broader national scrutiny (Engelberg and Parsons, 2011; Peress, 2014; Heese et al., 2022). Anecdotal evidence suggests that greenwashing is actively scrutinised by local media. For example, Shankman (2023) questioned

whether Greentown Labs, a Somerville-based clean-technology incubator, was engaging in greenwashing after partnering with major fossil-fuel companies such as Shell. The report raised concerns that such partnerships could enhance the climate-related credibility of oil firms without substantive changes in their underlying environmental practices. Importantly, this discussion did not remain confined to local coverage; it was subsequently reported by other traditional media outlets, including the Houston Chronicle, and sparked broader debate on social media platforms (Shankman, 2023; Drane, 2024). This example illustrates how local journalism can reveal discrepancies between environmental claims and actual practices. From this perspective, the closure of local newspapers may reduce external pressure for transparency, potentially leading to more pronounced greenwashing behavior.

At the same time, local newspapers may not always function as neutral arbiters. Their economic reliance on local advertisers may lead to selective coverage or favorable bias toward large employers or influential local firms (Shapira and Zingales, 2017; Gurun and Butler, 2012). To maintain subscriber loyalty and avoid alienating their readership base, local journalists may avoid negative reporting on major local firms (Shapira and Zingales, 2017). Moreover, Heese et al. (2022) document how limited staffing and investigative capacity can hinder the ability of local newspapers to uncover corporate misconduct. Thus, the actual disciplining role of local media may vary depending on context, leaving open the empirical question of whether newspaper closures systematically affect corporate greenwashing behavior.

This study examines the causal effect of local media scrutiny on corporate greenwashing by leveraging the staggered closure of local newspapers across U.S. counties between 2002 and 2017. The closure of local newspapers—driven primarily by industry-wide economic pressures rather than firm-specific environmental behavior—provides a plausibly exogenous shock to local information environments. This quasi-natural experiment allows us to evaluate how the withdrawal of journalistic oversight alters firms’ incentives to selectively disclose environmental performance. We examine greenwashing as a form of selective disclosure, de-

defined as the discrepancy between a firm’s symbolic environmental claims and its substantive operational actions (Hawn and Ioannou, 2016). Firms often emphasize relatively benign environmental indicators in external communications while omitting or obscuring more material shortcomings, thereby creating a misleading impression of environmental responsibility without necessarily breaching formal disclosure requirements (Delmas and Burbano, 2011; Lyon and Maxwell, 2011; Kim and Lyon, 2015; Bowen, 2010).

To estimate this effect, we implement a staggered difference-in-difference (DiD) framework comparing changes in greenwashing behavior between firms headquartered in counties that experienced newspaper closures and those in unaffected counties. The treatment variable equals one for firm-year observations in counties that experienced a newspaper closure when the observation year falls within the post-closure window from $t = +1$ to $t = +5$. This research design exploits cross-county variation in exposure to newspaper closures and within-firm variation over time, thereby allowing us to difference out time-invariant firm characteristics and common macroeconomic shocks. By benchmarking treated firms against firms located in counties without closures over the same period, the DiD framework facilitates a causal interpretation of the estimated treatment effect under the standard parallel trends assumption. Consistent with our hypotheses, we expect firms headquartered in counties experiencing newspaper closures to exhibit a differential change in greenwashing behavior in the post-closure period relative to firms in unaffected counties. Our main findings are consistent with this expectation. Specifically, we find that firms located in counties experiencing newspaper closures show a statistically significant increase in greenwashing behavior in the post-closure period relative to firms in unaffected counties. Beyond statistical significance, the estimated effect is also economically meaningful in magnitude, suggesting that the loss of local newspaper coverage leads to a substantive weakening of external monitoring and strategically expands firms’ scope to engage in greenwashing behavior.

To validate the robustness of the baseline findings, we conduct a series of additional tests. First, we augment the specification with county-level socioeconomic controls, including per

capita income, county population, population change, and the share of employment in the local labor force. After adding these variables, the treatment coefficient remains positive, statistically significant, and nearly unchanged relative to the baseline estimate (0.104 versus 0.109), while per capita income is positively associated with greenwashing. This suggests that regional socioeconomic conditions influence disclosure patterns but do not drive the estimated impact of newspaper closures. Second, we verify the parallel trends assumption by using the closure year ($t = 0$) as the benchmark category in a dynamic event-study specification. Pre-closure coefficients are small and insignificant, providing support for parallel trends. We also conduct placebo tests in which closure years are artificially shifted three years earlier. Consistent with expectations, no significant placebo effects are detected, reinforcing that the main results are not driven by spurious timing patterns. Third, we examine the sensitivity of the results to alternative definitions of the post-closure treatment window. Specifically, we re-estimate the static DiD model using four different windows: $[-4, -1]$ versus $[1, 4]$, $[-3, -1]$ versus $[1, 3]$, $[-5, -1]$ versus $[0, 4]$, and $[-4, 0]$ versus $[1, 5]$. Across all four specifications, the treatment effect remains positive and statistically significant. Fourth, we confirm that the findings are robust to alternative fixed-effects structures and to different operationalizations of greenwashing, including measures based on one-year-lagged components and Trucost-based disclosure ratios.

Additionally, heterogeneity analyses reveal systematic differences in firms' responses to the loss of local journalistic coverage, which help clarify the mechanisms through which newspaper closures affect greenwashing behavior. These cross-sectional patterns focus on variation in external monitoring and firms' incentives to respond when such monitoring weakens. Specifically, the analyses examine differences across firm size, community characteristics, alternative information channels, and firms' reporting incentives.

First, the effect of newspaper closures is significantly stronger among larger firms. Local newspapers play an important role in sustaining external oversight over highly visible firms, and their shutdown therefore leads to a larger decline in effective monitoring for these firms.

At the same time, large firms face stronger environmental and social expectations and thus have greater incentives to manage perceptions of their environmental performance. When local journalistic oversight weakens, this combination of strong incentives and increased opportunity allows large firms to benefit more from the reduced scrutiny, resulting in a more pronounced increase in greenwashing.

Second, the effect is concentrated in communities with lower levels of social capital. In these areas, community-based oversight and informal social monitoring are weaker, making local newspapers a particularly important source of external scrutiny. The disappearance of local journalistic coverage therefore results in a larger decline in effective monitoring, allowing firms greater room to engage in greenwashing.

Third, the effect is weaker in counties with higher residential internet usage, where online media can partially substitute for the monitoring role of local newspapers. In contrast, in areas with limited internet usage, local newspapers remain a primary channel through which external stakeholders observe and evaluate corporate behavior. Newspaper closures in these settings thus lead to a more pronounced reduction in external oversight.

Firms' internal reporting incentives also shape their disclosure responses to newspaper closures. The increase in greenwashing is concentrated among firms with stronger reporting incentives, as proxied by higher discretionary accruals, while firms with weaker incentives exhibit little response.

Finally, these disclosure responses have meaningful capital market consequences. Firms with higher pre-treatment greenwashing experience relatively more favorable valuation effects following newspaper closures, whereas firms with low pre-treatment greenwashing exhibit declines in market valuation, indicating that weakened local oversight affects not only disclosure behavior but also how capital markets price firms' environmental performance.

This paper makes several important contributions to the literatures on greenwashing and the informational role of local media.

First, this study contributes to the literature on local newspaper decline by examining

its implications for corporate environmental disclosure and communication. Prior research shows that local newspaper closures weaken external oversight and are associated with increases in real misconduct or environmental harm (Dyck et al., 2010; Campa, 2018; Jiang and Kong, 2023). However, much less is known about how the decline of local journalism affects firms’ ESG disclosure behavior and communication strategies. By focusing on greenwashing, the gap between firms’ symbolic environmental claims and their substantive environmental actions, this paper shows that the loss of local news coverage changes how firms present environmental performance to external stakeholders. In this way, the study complements existing work on real outcomes by highlighting a disclosure-based channel through which media decline affects perceived environmental performance.

Second, this study contributes to the greenwashing literature by providing causal evidence that greenwashing responds to changes in external monitoring. Much of the existing literature treats greenwashing as a firm-specific tendency driven by governance structures or managerial choices. In contrast, this paper shows that greenwashing increases following an exogenous reduction in local journalistic oversight, even after controlling for firm fundamentals. These findings suggest that greenwashing is shaped not only by internal firm characteristics but also by the strength of external scrutiny.

Third, this paper advances the broader understanding of how firms adjust their behavior in response to changes in external scrutiny by demonstrating how local news decline reshapes reputational incentives and symbolic accountability. Unlike prior work that views media coverage primarily as a monitoring channel (Heese et al., 2022), this study shows that reduced media visibility does not simply reduce disciplinary pressure but also alters firms’ strategic trade-offs in selectively disclosing environmental performance. As a result, the paper extends the literature on the broader consequences of local news decline, which has largely focused on civic participation and public governance (Gentzkow et al., 2011), to corporate ESG behavior. The findings indicate that the shutdown of local newspapers weakens symbolic accountability constraints, leading to higher levels of greenwashing.

Finally, the paper provides new evidence on the monitoring-based foundations of greenwashing through a set of heterogeneity analyses. The effects of newspaper closures are stronger among larger firms, in communities with lower social capital, in areas with lower internet usage, and among firms with stronger reporting incentives. These patterns indicate that local newspapers play an important role in supporting external oversight. When this oversight weakens, firms that are more visible or more willing to adjust disclosure and communication strategies respond more strongly. Moreover, the paper shows that these disclosure responses are associated with meaningful capital market consequences: firms with higher pre-treatment greenwashing experience relatively more favorable valuation outcomes following newspaper closures, while firms with low pre-treatment greenwashing exhibit declines in valuation. Overall, the findings help clarify when and where local journalism matters most for limiting greenwashing and promoting environmental transparency.

Taken together, these contributions clarify the informational foundations of greenwashing and underscore the critical role of local journalism in shaping corporate environmental accountability. As ESG disclosure remains largely voluntary and reputationally driven, the findings highlight an important externality of local media decline: weakened journalistic oversight not only affects real environmental behavior but also facilitates symbolic compliance that heightens perceived greenwashing risk and intensifies CSR decoupling.

2 Literature Review

2.1 Corporate Greenwashing

Environmental, social, and governance (ESG) concerns have emerged as a central theme in corporate strategy and financial markets, particularly following the adoption of the United Nations Sustainable Development Goals (SDGs) in 2015 and the signing of the Paris Agreement in the same year (United Nations, 2015; United Nations Framework Convention on Climate Change, 2015). These international frameworks have heightened pressure on firms to

demonstrate environmental responsibility and align with sustainability expectations. However, the increasing emphasis on sustainability has also created new incentives for greenwashing, which refers to the act of projecting a false image of compliance without undertaking substantive environmental actions. Understanding this tension is essential for evaluating the credibility of ESG disclosures.

The term greenwashing was first coined by environmentalist Jay Westervelt in 1986 to describe misleading environmental messaging in the hotel industry (de Freitas Netto et al., 2020). While early studies focused on product-level deception, such as exaggerated claims about recyclable packaging or energy efficiency, recent literature has shifted toward examining greenwashing at the firm level. In this broader context, greenwashing reflects a strategic misalignment between firms’ external environmental claims and their internal operational practices (Habek and Wolniak, 2015).

According to Hawn and Ioannou (2016), greenwashing is defined as the discrepancy between a firm’s “green talk” and “green walk.” The former refers to symbolic ESG communications, including sustainability reports, press releases, or marketing campaigns, whereas the latter captures substantive ESG activities, such as environmental investments, emissions reductions, and compliance measures. The gap between these two dimensions forms the conceptual foundation for measuring greenwashing and is often operationalized as the difference between symbolic and substantive ESG indicators.

Recent research suggests that ESG disclosure is frequently used in a symbolic manner to enhance legitimacy or attract capital, even when actual environmental improvements are minimal or nonexistent (Cho et al., 2012; Romero et al., 2019). Berrone et al. (2017); García-Sánchez and Martínez-Ferrero (2018) argue that firms often respond to stakeholder pressure or reputational concerns by overstating their commitment to sustainability. This symbolic use of ESG language is often driven by rational cost-benefit considerations, particularly in regulatory environments where enforcement is weak but reputational returns are high.

Empirical evidence confirms the widespread nature of greenwashing. According to a

2020 global screening by the International Consumer Protection and Enforcement Network (ICPEN), 42 percent of corporate environmental claims were found to be exaggerated, false, or misleading (International Consumer Protection and Enforcement Network, 2020). Misrepresentation is particularly common in consumer-facing sectors such as fashion, cosmetics, and household goods, where marketing plays a central role in shaping public perception. Firms frequently portray themselves as environmentally responsible while offering insufficient verifiable evidence to support their claims.

A well-known real-world example is the Volkswagen emissions scandal. In 2009, Volkswagen began selling over 480,000 “clean diesel” vehicles in the United States. However, it was later revealed that the company had installed software that activated pollution controls only during laboratory tests, while disabling them during normal driving conditions. As a result, the vehicles emitted pollutants at levels up to 40 times higher than legal limits. In 2015, the U.S. government fined Volkswagen \$14.7 billion for violating emissions laws and misleading consumers. This case exemplifies how firms may use green branding to conceal environmentally harmful behavior (Speculations, 2015).

Firms’ incentives to engage in greenwashing are shaped by a variety of conflicting forces. On one hand, they may benefit from enhanced ESG reputation, preferential loan terms, and long-term access to green finance (He et al., 2019; Yang et al., 2017; Yu et al., 2020). On the other hand, they face substantial obstacles, such as high costs of innovation, challenges in monitoring, and the potential for reputational damage or legal consequences if their greenwashing is uncovered (Petitjean, 2019; Zhang, 2021). This tension illustrates the fundamental trade-off that firms often face between sustainability and profit maximization (Friedman, 1970).

The credibility of ESG disclosures is further undermined by the absence of standardized reporting frameworks. Compared to traditional financial reporting, ESG disclosures often suffer from low levels of accuracy, transparency, and comparability (Del Giudice and Rigamonti, 2020). The non-financial nature of ESG data, coupled with inconsistencies in

global disclosure standards, provides firms with considerable discretion to selectively present favorable information. For example, King and Lenox (2001) show that firms may exaggerate environmental progress in order to gain competitive advantage.

In the absence of credible third-party auditing or strong regulatory enforcement, media scrutiny plays a vital role in constraining opportunistic ESG reporting. By disseminating corporate environmental claims to broader audiences, the media reduces information asymmetry, enhances accountability, and imposes reputational consequences on misrepresentation, which serves as a deterrent to greenwashing and strengthens corporate transparency and discipline (Dyck et al., 2008; Miller, 2006; Dyck et al., 2010). Through investigative reporting, media outlets can expose discrepancies between firms’ public disclosures and actual practices, thereby limiting the scope for opportunistic behavior. As Dyck et al. (2008) note, the media helps lower transaction costs by aggregating and distributing information to the public. Miller (2006) further argues that growing consumer demand for environmental responsibility intensifies media scrutiny, which can hold firms accountable for unsustainable practices and discourage symbolic disclosures not backed by real actions.

Nevertheless, the effectiveness of this mechanism depends on the strength of the media landscape. When media oversight declines, particularly in local contexts where information gaps are most severe, it remains unclear how corporate behavior will adjust in response. This uncertainty underscores the importance of understanding the consequences of weakening media environments for ESG credibility.

2.2 Local Newspaper Closures

While local newspapers serve as important monitors of corporate behavior, their independence can be compromised by financial dependencies. Approximately 60% of their revenue comes from local advertisers (Gurun and Butler, 2012), which may lead to positive reporting bias and reluctance to criticize key sponsors. Newspapers also rely on community subscriptions, creating additional pressure to avoid content that could alienate readers—many

of whom are employed by the very firms under scrutiny (Mullainathan and Shleifer, 2005; Shapira and Zingales, 2017). Despite these limitations, local newspapers still play a meaningful role in monitoring corporate behavior and reducing information asymmetry, particularly in settings where external enforcement mechanisms are weak or inaccessible (Ma et al., 2022).

Media scrutiny deters greenwashing. Media scrutiny discourages symbolic disclosure, and regulatory institutions can further discourage such practices by imposing oversight (Kim and Lyon, 2015). Media organizations enhance transparency by aggregating and publicising corporate information, thereby reducing transaction costs and promoting accountability (Dyck and Zingales, 2005; Miller, 2006).

Newspapers act as essential watchdogs. For example, coverage by the Financial Times and the Wall Street Journal of self-dealing by Russian firms significantly reduced governance violations by damaging reputations and triggering regulatory investigations (Dyck et al., 2008). Similarly, Snyder and Strömberg (2010) argue that local newspapers, by informing citizens, may also enhance firms’ environmental responsibility. Empirical evidence shows that newspaper closures lead to more corporate misconduct, as firms exploit the loss of scrutiny, confirming that media attention deters unethical behavior by increasing reputational risk (Heese et al., 2022; Dyck et al., 2010).

Due to their proximity and access to local networks, local newspapers are often better positioned than national outlets to detect greenwashing at the community level. Their familiarity with regional industries, workforce conditions, and stakeholder dynamics enables them to identify discrepancies in corporate sustainability claims that might otherwise go unnoticed (Miller and Shanthikumar, 2011; Shapira and Zingales, 2017). This localized vigilance is critical for maintaining transparency and accountability in corporate environmental communication.

Secondly, local newspapers act as external monitors for local firms. Miller (2006) documents that the national press monitors corporations for their wrongdoings by providing information about accounting fraud. Miller and Shanthikumar (2011) provide evidence that

local newspapers often cover more local firms than national newspapers. Local newspapers can supply the national press with evidence of firms' fraud and misconduct, further disseminating this information. Therefore, we expect that the closure of a local newspaper will weaken its monitoring role on local firms, increasing their chances of performing greenwashing.

Building on this literature, we examine whether the closure of local newspapers influences firms' propensity to engage in greenwashing. Local newspapers frequently cover corporate sustainability claims and may serve as the first line of journalistic oversight, particularly in communities where other accountability institutions are limited. Their disappearance removes a layer of public visibility and may alter how firms approach ESG disclosure. As Miller (2006) point out, local newspapers are often better positioned to report on nearby firms due to their proximity and access to community resources. Ma et al. (2022) estimate that more than 50% of original local and regional news in the U.S. is generated by local newspapers. When these outlets shut down, the reach and depth of environmental disclosure coverage is likely to decline, weakening the broader media infrastructure that supports transparency.

The decline of local newspapers has become a critical issue with far-reaching implications for corporate transparency and accountability. Traditionally, local newspapers have played an essential role in monitoring business conduct by providing timely and localized coverage of corporate activities. Their reporting helps shed light on firms' environmental behavior and disseminates corporate disclosures to broader audiences. Research by Heese et al. (2022) finds that newspaper closures are associated with increased corporate misconduct, suggesting that firms take advantage of diminished scrutiny. Similarly, Dyck et al. (2010) demonstrate that media attention deters unethical behavior by elevating reputational risks and exposing malpractices to public view.

Moreover, the disappearance of local newspapers is likely to diminish the quality and scope of information available to stakeholders. National media often lacks the proximity and depth to report on localized issues with the same immediacy (Lyon and Maxwell, 2011).

The loss of localized investigative capacity—essential for uncovering complex or context-specific corporate behavior—can reduce the likelihood that firms’ symbolic ESG disclosures are scrutinized or contested. As Schiffrin (2014) notes, meaningful investigative journalism typically requires sustained local engagement and specialized knowledge that national outlets may not consistently provide.

However, the decline in media visibility following newspaper closures may also reshape the incentives underlying greenwashing. Media coverage amplifies the reputational value of ESG disclosures by broadcasting them to wider audiences and enhancing stakeholder scrutiny (Dyck et al., 2010). In the absence of this visibility, firms may perceive fewer benefits from symbolic disclosure, as their environmental messaging is less likely to reach or influence key audiences.

At the same time, the reduction in journalistic oversight may decrease the expected cost of misrepresentation. With fewer investigations into inconsistencies between reported and actual environmental performance, firms may face less reputational or regulatory risk for engaging in greenwashing (Jiang and Kong, 2023; Schiffrin, 2014). As a result, greenwashing behavior may increase due to weaker deterrence, or decline due to muted reputational rewards.

Building on these insights, this study examines whether the loss of local newspaper coverage affects corporate greenwashing behavior. On the one hand, closures reduce external scrutiny from the media, potentially enabling firms to overstate their environmental performance. On the other hand, diminished media visibility may weaken the reputational incentives for symbolic disclosure.

The net effect of these opposing forces remains an open empirical question.

H1: Local newspaper closures affect corporate greenwashing behavior by simultaneously reducing external oversight and reputational incentives. We hypothesize that the former effect dominates, such that newspaper closures lead to an increase in greenwashing.

3 Data

3.1 Sample

Sample. Table 1 summarizes the construction of the final analysis sample. The starting point is the panel of United States publicly listed firms from WRDS Compustat, which provides 178,380 firm year observations between 2002 and 2017 for 22,895 unique firms in 73 two digit SIC industries.

In the first step, these observations are merged with Thomson Reuters’ Refinitiv ASSET4 database to obtain firm-year measures of greenwashing based on firms’ sustainable disclosure and performance.

Corporate greenwashing is conceptualized as the disparity between a firm’s symbolic and substantive environmental actions as reflected in CSR-related disclosure (Hawn and Ioannou, 2016; Bothello et al., 2023). Substantive environmental actions capture changes in firms’ core practices, organizational norms, structures, and long-term investments undertaken to address environmental risks that primarily affect internal stakeholders such as employees, managers, and owners, while symbolic environmental actions refer to how firms communicate, frame, and describe these internal efforts to external stakeholders.

While both substantive and symbolic actions can enhance firms’ legitimacy and public image symbolic communication alone does not constitute greenwashing. Greenwashing arises when a pronounced misalignment between the two indicates that external claims overstate underlying environmental efforts. Such a gap reflects a tendency to promote an environmentally responsible image that is not fully supported by actual practices and is therefore interpreted by stakeholders as greenwashing. Consistent with this interpretation, prior studies show that stakeholders view larger symbolic–substantive gaps as indicative of greenwashing risk and respond negatively, with evidence from capital markets and real outcomes indicating that firms exhibiting greater misalignment are penalized by investors and other stakeholders.

In conclusion, greenwashing arises when a firm’s reported actions do not align with its

real environmental efforts. Thomson Reuters' Refinitiv environmental data are classified into Substantive and Symbolic CSR Indices' Items, distinguishing between internal environmental practices and external actions (Hawn and Ioannou, 2016; Bothello et al., 2023; Hussain et al., 2023).¹

Earlier research has used two primary sources to document newspaper closures. Darr et al. (2018) relied on the Chronicling America database, managed by the Library of Congress, which records the founding and closing dates of thousands of newspapers. Gentzkow et al. (2011) and Gao et al. (2020) utilized the Editor & Publisher Yearbook, an annual publication that provides a comprehensive overview of U.S. newspapers, including information on publication frequency and geographic coverage. These studies typically define a newspaper closure as a drop in the number of daily newspapers within a county from one year to the next, and assign newspapers to counties using data from the 2010 U.S. Census. Building on this literature, we draw on Jiang and Kong (2023), who combine both sources using these matching procedures to construct a comprehensive list of daily newspaper closures. We adopt their Appendix Table A1, which reports 236 closures between 2000 and 2018, and following their approach of retaining only the first closure event in counties with multiple closures and restricting to our sample period, we obtain a final set of 159 county-level newspaper closures between 2002 and 2017.

For population data, we follow the methodologies outlined by Jiang and Kong (2023) and Gao et al. (2020). Annual population and per capita income data are sourced from the U.S. Bureau of Economic Analysis (BEA) through the CAINC1 dataset. This dataset provides county-level population estimates, which are based on the U.S. Census Bureau's midyear estimates and adjusted using the intercensal method to account for decennial Census revisions. Additionally, employment data are gathered from the BEA's CAINC4 dataset,

¹For example, reporting initiatives to reduce emissions or to promote environmentally friendly products reflects a symbolic, externally oriented action aimed at shaping stakeholders' perceptions, as captured by disclosure-based items such as whether the company reports initiatives to reduce or compensate CO₂ emissions, whereas adopting a formal policy to reduce emissions represents a substantive, internal action that reflects concrete organizational commitments and changes in operational practices. Appendix A2 provides the detailed classification of all items used in this study.

which includes annual employment and wage data at the county level. Employment figures are normalized by dividing total employment by the county’s population for each year. These data allow us to control for key macroeconomic conditions at the county level, ensuring that fluctuations in population size and employment are accounted for, which are factors that could influence both newspaper closures and corporate greenwashing behavior.

Having assembled the county-level information on newspaper closures and demographic characteristics, we next match firms to their historical headquarters counties using information extracted from the text of Form 10-K filings. This step yields 15,223 firm-year observations for 2,701 firms and provides the necessary link for merging firm-level data with the corresponding county-level newspaper closure indicators and population-based socioeconomic variables.

Finally, we remove observations with missing values in any variables required for the baseline regressions, including standard firm level controls and county level demographic variables. After this filtering process, the final estimation sample contains 9,226 firm year observations for 1,566 firms in 65 industries during the period 2002 to 2017. This dataset forms the basis of the baseline analysis and all subsequent heterogeneity tests.

3.2 Empirical Methodology and Sample Distribution

Empirical Methodology We examine the effect of local newspaper closures on corporate greenwashing using a difference-in-differences approach. The main regression estimated in this study is:

$$GW_{ict} = \beta (Closure_c \times Post_t) + Controls_{i,t} + \alpha_i + \gamma_{st} + \varepsilon_{ict} \quad (1)$$

where the dependent variable GW_{ict} is the greenwashing score of firm i , headquartered in county c , in year t . The variable $(Closure_c \times Post_t)$ is an indicator that equals one for firm-years in counties that experienced a newspaper closure and that fall within the post-

closure window from event years $t = +1$ to $t = +5$, and it equals zero for all pre-closure years from $t = -5$ to $t = -1$ as well as for all firms in counties without a closure.² The closure year itself, $t = 0$, is excluded to avoid confounding anticipatory behavior or transitional adjustments that may occur during the event year (Jiang and Kong, 2023).

This identification strategy allows us to implement a difference-in-differences design that exploits the staggered timing of newspaper closures across counties. In our setting, the treated group consists of firms located in counties that experience a newspaper closure, observed in the post-closure period. The control group consists of (i) firms in the same treated counties before the closure and (ii) firms located in counties that never experience a closure during our sample period. The first difference compares changes in greenwashing for treated firms before and after the closure of a local newspaper. The second difference compares these changes with firms located in counties without a closure. The estimated coefficient β therefore captures the average change in greenwashing attributable to the loss of local news coverage.

Firm-level controls $Controls_{i,t}$ include size, profitability, fixed assets, research and development, leverage, capital expenditures scaled by total assets, and risk. All continuous control variables are winsorized at the 1% and 99% levels. Variable definitions are provided in the Appendix A1.

We include firm fixed effects α_i to control for time-invariant firm characteristics and state-by-year fixed effects γ_{st} to absorb regional macroeconomic conditions and shocks. This empirical structure ensures that the treatment effect is identified from variation in closure timing across counties within the same state and year, while also accounting for local and firm-specific shocks. Firm fixed effects capture baseline levels of symbolic environmental disclosure, and state-by-year fixed effects absorb changes in disclosure practices within each

²Alternative classifications of the event year may also be reasonable, following the discussion in Jiang and Kong (2023). If firm managers require time to become aware of the closure, the event year may be allocated to the pre-period (for example, $[-4, 0]$ versus $[1, 5]$). Conversely, if the closure is widely anticipated in the local community, the event year may be included in the post-period (for example, $[-5, -1]$ versus $[0, 4]$). As shown in the robustness analysis, these alternative specifications yield consistent inferences.

state over time that are unrelated to newspaper closures. Standard errors are clustered at the state-year level to address within-cluster correlation in both treatment timing and local economic environments.

Following prior studies (e.g., Marquis et al. (2016); Berrone et al. (2017); Gao et al. (2020); Arouri et al. (2021); Mateo-Márquez et al. (2022); Zhang (2023); Jiang and Kong (2023)), we control for a comprehensive set of firm- and county-level characteristics that may affect greenwashing behavior and are potentially associated with local newspaper closures. Prior research suggests that firm size influences both stakeholder attention and the capacity to manage external perceptions. Larger firms tend to receive more media coverage, are embedded in broader networks, and are more likely to be subject to external scrutiny (Reitz et al., 1979; Singh, 1986). In this study, we measure firm size using the natural logarithm of total employees. This approach follows prior literature showing that employees represent a powerful stakeholder group capable of influencing corporate decisions, and that large employers may wield disproportionate political and social influence within a country (Berrone et al., 2017). This specification allows us to capture the dual effects of firm scale and stakeholder dynamics on corporate disclosure behavior.

According to Delmas and Burbano (2011), firms' size and profitability can shape their propensity to adopt greenwashing strategies. Firm performance is measured by return on assets, calculated as net income divided by total assets. More profitable firms are likely to be more operationally efficient and able to allocate resources to environmental or symbolic strategies (Dowling and Pfeffer, 1975; Meyer and Rowan, 1977). Fixed Asset, also known as capital intensity, is measured as net property, plant, and equipment scaled by total assets (Marquis et al., 2016; Arouri et al., 2021; Jiang and Kong, 2023). This variable reflects a firm's potential to cause environmental impact and the likelihood of scrutiny over environmental performance. In addition, capital intensity can capture important intra-industry variation in operational structure and disclosure practices. To account for firms' financing needs, we control for the debt ratio (total debt over assets). Higher leverage often implies

greater pressure from creditors, which may incentivize firms to avoid disclosing negative environmental performance (Zhang, 2023; Tian et al., 2025; Kim et al., 2021). We also include capital expenditures scaled by total assets as a proxy for long-term strategic investment, which may relate to strategic disclosure. Firms engaging in larger capital outlays may be more inclined to shape stakeholder perceptions through disclosure practices. Firm-level risk is further measured by the inverse of the debt-to-equity ratio, capturing firms' solvency. Prior literature shows that financially constrained or distressed firms may strategically use symbolic disclosure to maintain legitimacy under external pressure (Mateo-Márquez et al., 2022).

At the county level, in line with Gentzkow et al. (2011); Gao et al. (2020); Jiang and Kong (2023), we include income per capita (expressed as the natural logarithm of county income per capita), county population (measured as the natural logarithm of population for each county-year), population change (the percentage change in population from the previous year) and employment change (county annual employment deflated by population in each year) to account for the macroeconomic conditions in each county. Echoing the insights of Gentzkow et al. (2011), we include population size and per capita income due to their historical significance in determining newspaper market dynamics, thereby safeguarding our study against potential confounders that could be related to both newspaper closures and corporate greenwashing. These county-level economic indicators are also crucial in excluding the possibility that the economic conditions of a county are driving both the shutdown of its newspapers and the greenwashing practices of local firms.

Further, prior literature emphasizes the role of economic development in shaping environmental expectations. Some scholars (De Soto, 1989; Husted, 2005) argue that wealthier and more developed regions are more likely to demand environmental accountability and foster sustainability. For example, Grant and Gnyawali (1996) finds that individuals in economically advanced areas are better informed and more likely to pressure companies for responsible behavior. However, other studies (Chapple and Moon, 2005; Ioannou and Ser-

afeim, 2012) highlight that economic development is not the sole determinant of corporate social performance across regions, as variations may also arise from differences in institutional frameworks, cultural norms, and information environments.

Sample Distribution Table 2 and Figure 1 together provide an overview of the timing and distribution of local newspaper closures and the resulting treatment exposure in our firm-year sample. Figure 1 plots the annual number of verified local newspaper closures from 2002 to 2017, based on the dataset constructed by Jiang and Kong (2023). The figure shows substantial temporal variation, with relatively few closures in the early 2000s, a pronounced spike in 2009, and a sharp rise beginning in 2014. The number of closures peaks at 31 in 2015 before declining thereafter. These patterns reflect the broader decline of local news institutions during this period and generate the staggered treatment timing exploited in our analysis.

Table 2 reports the corresponding yearly distribution of treated and control firm-year observations. The total number of firm-year observations increases steadily over time, rising from 121 in 2002 to over 1,200 in both 2016 and 2017, as more firms meet the data requirements in later years. Consistent with the rising number of newspaper closures, the count of treated observations also grows over time, increasing from 15 in 2002 to 209 in 2017. Although treated observations constitute a relatively small share of the sample in any given year (approximately 10–20 percent), their steady expansion mirrors the pattern of closures shown in Figure 1.

Taken together, the figure and table illustrate considerable temporal heterogeneity in the incidence of newspaper closures and the corresponding exposure of firms to the treatment. This variation underpins our difference-in-differences identification strategy, which relies on comparing firms in counties that experience closures at different points in time with those in counties that do not experience closures.

Summary Statistics The summary statistics in Table 3 provide an overview of the key variables used in the analysis. The full sample consists of 9,226 firm-year observations from 2002 to 2017.

A comparison between firms headquartered in counties that experience a newspaper closure and those in non-closure counties reveals meaningful differences in greenwashing behavior. Firms in closure counties exhibit slightly higher levels of greenwashing on average, with a mean of -2.458 compared with -2.449 for firms in non-closure counties, and a median of -2.484 versus -2.482 , respectively. Although the numerical differences are modest, the results indicate that firms located in counties that experience a newspaper closure engage in more greenwashing relative to those in non-closure counties.

Beyond greenwashing, closure-county firms also differ along several firm- and county-level dimensions. When firm size is measured as the natural logarithm of total employees, treated firms appear smaller on average. They also report marginally higher profitability and risk and operate in counties with larger populations, higher income levels, and slightly stronger population growth. These systematic differences underscore the importance of including firm and state-year fixed effects in the empirical design to account for underlying heterogeneity across both firms and local economic environments.³

4 Results

4.1 Baseline Specification

Table 4 reports the baseline estimates of the effect of local newspaper closures on corporate greenwashing, using firm-level controls and absorbing firm and state-year fixed effects. Column (1) shows that the coefficient on *Treatment* is positive and statistically significant at the 1% level (0.104 , $p < 0.01$), indicating that firms headquartered in counties that ex-

³If firm size is instead measured using the natural logarithm of total assets, firms in closure counties are larger on average (Mean = 8.564 , Median = 8.412 , SD = 1.364) than those in non-closure counties (Mean = 8.489 , Median = 8.400 , SD = 1.558).

perienced a newspaper closure are more likely to engage in greenwashing. This finding is consistent with the interpretation that the disappearance of local newspaper coverage reduces the availability of credible local information, weakens external scrutiny, and increases information asymmetry. In such environments, firms face fewer reputational and monitoring constraints and thus have stronger incentives to exaggerate their environmental commitment through symbolic disclosure rather than substantive improvements. (Miller, 2006; Dyck et al., 2008; Di Giuli and Kostovetsky, 2014; Hoi et al., 2018; Albuquerque et al., 2019; Gao et al., 2020; Heese et al., 2022; Kyung and Nam, 2023; Jiang and Kong, 2023).

Among the firm-level characteristics, *Firm Size* enters with a positive and highly significant coefficient (0.120, $p < 0.01$), indicating that larger firms are more likely to engage in greenwashing. This pattern aligns with prior research showing that larger and more visible firms attract greater media attention and are subject to closer monitoring (Miller, 2006). As noted by Heese et al. (2022), the visibility of large firms implies that the reduction in local information dissemination caused by newspaper closures disproportionately affects them, weakening an important channel through which misconduct or symbolic behavior would otherwise be revealed. At the same time, larger firms face heightened expectations from stakeholders to maintain an environmentally responsible public image. The pressure to preserve social legitimacy may lead these firms to adopt symbolic disclosure strategies, particularly when substantive environmental improvements are costly or slow to materialize (Marquis et al., 2016; Berrone et al., 2017; Zhang, 2023). When local journalistic scrutiny diminishes following a newspaper closure, the incentives for large firms to rely on such symbolic claims become even stronger.

The coefficient on *Fixed Asset* is negative and marginally significant (-0.220 , $p < 0.10$), suggesting that capital-intensive firms are less likely to engage in greenwashing. This interpretation is consistent with prior work noting that capital intensity can affect both the extent of environmental damage a firm may generate and the likelihood of selective disclosure (Marquis et al., 2016; Arouri et al., 2021). Firms with a larger share of resources

committed to property, plant, and equipment typically face operational and technological constraints that tie their environmental performance more directly to underlying production processes, limiting the scope for symbolic claims that deviate from actual environmental outcomes. In addition, capital intensity captures important intra-industry variation in production technologies and operating characteristics (Marquis et al., 2016), which influences firms' disclosure incentives and reduces their ability to rely on purely symbolic environmental statements. These considerations help explain why capital-intensive firms in our sample exhibit lower levels of greenwashing.

The coefficient on *R&D* is negative and statistically significant (-0.652 , $p < 0.05$), indicating that firms with greater innovation intensity are less likely to engage in greenwashing. Prior literature shows that R&D intensity is positively associated with agency problems and monitoring cost (Himmelberg et al., 1999; Lee et al., 2008). In this context, a negative association suggests that firms with higher monitoring costs face stronger market discipline, leaving them with less room to rely on purely symbolic environmental statement. In addition, R&D-intensive firms often pursue longer-term strategic orientations and invest in technological capabilities, making substantive environmental improvements more aligned with their innovation-driven strategies. As a result, these firms have fewer incentives to engage in symbolic disclosure.

The *Risk* variable is positive and significant (0.021 , $p < 0.05$), indicating that firms with higher risk exposure are more likely to engage in greenwashing. This result is consistent with Roulet and Touboul (2015); Mateo-Márquez et al. (2022), who argue that firms facing greater risk have stronger incentives to resort to symbolic environmental claims.

After accounting for firm and state-year fixed effects, variables such as *ROA*, *Debt Ratio*, and *Capital Expenditure* do not show statistically significant associations with greenwashing, indicating that short-term profitability and financing structure are not the primary drivers of symbolic disclosure in this setting. When county-level characteristics including *Ln Income per Capita*, *Ln County Population*, *%Population Change* and *%Employment* are added, the

main findings do not change. Only *Ln Income per Capita* becomes marginally significant, while the other county-level variables remain insignificant. Most importantly, the coefficient on *Treatment* remains stable in both magnitude and significance, confirming that the estimated effect of newspaper closures is not driven by unobserved regional socioeconomic conditions. The persistence of this effect after incorporating county-level controls strengthens confidence that the results are not explained by local confounders. Overall, the positive and significant impact of newspaper closures on greenwashing is robust to the inclusion of both firm-level and county-level controls and provides a reliable basis for the subsequent heterogeneity analyses.

4.2 Robustness tests

Parallel Trend. To examine how the effect of local newspaper closures evolves over time, I estimate a dynamic event-study specification in which the year of the closure ($t = 0$) serves as the benchmark category. Indicators for the five years before and the five years after the closure are included, and firm and state-year fixed effects are absorbed. The coefficients therefore capture deviations in greenwashing relative to the closure year.

Table 5 reports the estimated coefficients. Across all pre-closure periods, the coefficients are small in magnitude and statistically indistinguishable from zero, indicating that treated and control firms exhibit parallel trends prior to the disappearance of local journalistic coverage.

Following the closure, however, the pattern changes markedly. The dynamic coefficients begin to rise in the second year after the event, with statistically significant increases in greenwashing appearing in years $t = +2$, $t = +3$, and $t = +4$. This trajectory suggests that the weakening of the local information environment does not generate an immediate shift in firms' disclosure behavior. Instead, as the informational effects of the closure accumulate and external scrutiny continues to decline, firms gradually increase their reliance on symbolic environmental claims. The effect reaches its peak several years after the closure, consistent

with a slow-moving adjustment process in which managers respond incrementally to declining media oversight.

In year $t = +5$, the magnitude of the estimate decreases relative to its peak, indicating a partial attenuation of the effect. This suggests that the impact of newspaper closures on greenwashing may diminish over longer horizons, either because stakeholders adapt to the new information environment, or because firms exhaust the scope for symbolic disclosure without substantive improvement. Taken together, these dynamic estimates provide further evidence that local newspaper closures lead to increased greenwashing, with the effect intensifying as the informational environment deteriorates and subsequently showing signs of moderation.

Placebo Test. To further assess the validity of the parallel trends assumption and rule out spurious correlations, I conduct a placebo test, following Roberts and Whited (2013) and Jiang et al. (2016). Specifically, I artificially shift each newspaper closure three years earlier than its actual timing, reconstruct the event-time structure using this pseudo closure year, and re-estimate the baseline specification using the same five-year symmetric window and exclusion rules as in the main analysis.

Table 6 presents the results. The estimated coefficient on the placebo treatment is small in magnitude and statistically insignificant, indicating that artificially advancing the closure year does not generate detectable changes in firms' greenwashing behavior.

This falsification exercise provides additional evidence that the observed treatment effect is driven by actual newspaper closures rather than underlying differential trends between treatment and control firms.

Varying the Length of the Event Window and Treatment Definition. To examine whether the baseline findings are sensitive to the construction of the event window or the classification of the closure year, I re-estimate the core specification using several alternative windows around the newspaper closure year, as reported in Panel A of Table 7.

First, Columns (1) and (2) implement symmetric windows of different lengths while consistently excluding the closure year to maintain clean identification. Column (1) uses the window $[-4, -1]$ and $[1, 4]$, and Column (2) applies $[-3, -1]$ and $[1, 3]$. Across both specifications, the estimated treatment effects remain positive and statistically significant, indicating that the results are not driven by a particular choice of window length when the closure year is excluded.

Second, Columns (3) and (4) modify the definition of the treatment period by allowing the closure year to be allocated either to the pre-period or to the post-period, following the arguments discussed in Jiang and Kong (2024). If firm managers require time to become aware of the newspaper closure, it may be more appropriate to classify the closure year into the pre-period. Column (3) therefore uses the window $[-5, -1]$ and $[0, 4]$. Conversely, if the closure of a local newspaper is widely anticipated in the community, it may be reasonable to classify the closure year into the post-period. Column (4) adopts the window $[-4, -1]$ and $[0, 3]$ under this alternative assumption. Across both specifications, the estimated treatment effects remain positive and significant, suggesting that the findings are not sensitive to how the closure year is allocated.

In both cases, the treatment effects remain positive and statistically significant, demonstrating that the estimated impact of newspaper closures on greenwashing is not sensitive to how the closure year is allocated between the pre- and post-periods.

Taken together, the consistency in sign, magnitude, and statistical significance across all four specifications indicates that the estimated effects are robust to both the length of the event window and the treatment definition. This strengthens the conclusion that local newspaper closures increase firms' greenwashing behavior rather than reflecting artifacts of window choice or coding rules.

Alternative Fixed Effects Specifications. The choice of fixed effects is crucial for obtaining credible difference-in-differences estimates. Because newspaper closures vary at the

county level and may coincide with broader regional developments, the baseline specification employs firm fixed effects and state-by-year fixed effects. Firm fixed effects absorb all time-invariant differences across firms, while state-by-year fixed effects flexibly capture local economic, regulatory, or informational conditions that change over time within each state.

Panel B of Table 7 compares alternative fixed-effects specifications. Using only firm fixed effects (column 1) yields a smaller treatment effect, whereas adding state-by-year fixed effects (column 2) increases the coefficient on *Treatment* to 0.104 ($p < 0.01$), highlighting the importance of controlling for regional time-varying shocks. Column (3), which includes firm fixed effects and industry-by-year fixed effects, produces a treatment effect of 0.062 ($p < 0.05$), indicating that sector-specific trends also matter but do not fully substitute for regional controls. The most saturated model (column 4), combining firm, state-by-year, and industry-by-year fixed effects, yields the largest estimate (0.130, $p < 0.01$), suggesting that both regional and industry time-varying heterogeneity influence firms' disclosure incentives.

Taken together, these comparisons justify the baseline choice of firm fixed effects and state-by-year fixed effects, which effectively absorb key sources of unobserved heterogeneity while maintaining estimation precision.

Alternative Greenwashing Measurement. To assess whether the baseline findings depend on how greenwashing is defined, I re-estimate the main specification using three alternative measures widely used in the literature. The first measure, GW (column 1), is the benchmark symbolic–substantive gap following Hawn and Ioannou (2016), constructed using symbolic and substantial CSR disclosures from the same fiscal year.

The second measure, GW (one-year lag) (column 2), captures a staggered symbolic–substantive mismatch in which substantial CSR items are measured in year $t - 1$ and symbolic CSR disclosures are measured in year t . This construction reflects the idea that firms may strategically issue symbolic claims after observing the outcomes of prior substantive CSR activities, creating a lagged form of greenwashing that has been emphasized in prior research.

The third measure, GW Trucost (column 3), follows Marquis et al. (2016) and captures selective environmental disclosure using Trucost-based emissions and reporting data, focusing on the inconsistency between environmental transparency and actual environmental performance.

Across all three measures, the estimated treatment effects remain positive and statistically significant, as shown in Panel C of Table 7. The effect sizes for the benchmark GW measure and the lagged GW measure are similar (0.104 and 0.117, both significant at the 1 percent level), indicating that the association between newspaper closures and greenwashing is robust to contemporaneous versus lagged constructions of symbolic disclosure. The Trucost-based measure also yields a positive and significant coefficient (2.274, $p < 0.10$), despite its smaller sample and conceptual differences.

Taken together, these results demonstrate that the main conclusion is robust to alternative operationalizations of greenwashing. Whether measured through within-year symbolic-substantive inconsistency or a lagged symbolic response to prior substantive actions, or through selective environmental transparency, firms exhibit significantly higher levels of greenwashing following local newspaper closures.

4.3 Additional Tests

To further explore the mechanisms through which local newspaper closures influence greenwashing, we conduct a series of cross-sectional analyses examining whether the estimated treatment effects vary systematically across different monitoring and incentive environments. We begin by examining firm size, which captures corporate visibility and exposure to external monitoring. We then study county-level social capital and residential internet usage, which reflect the strength of community-based oversight and the availability of alternative online information channels, respectively. Next, we analyze firms' reporting incentives using discretionary accruals (DACC), which proxy for managers' incentives and discretion in strategic disclosure. Finally, we examine capital market consequences to assess how these

changes in monitoring and incentives are reflected in market valuation. Overall, these analyses help clarify the conditions under which local newspapers serve as a particularly important source of external monitoring and scrutiny, thereby shaping firms' incentives to engage in greenwashing.

Heterogeneity by Firm Size Following prior research such as Miller (2006) and Heese et al. (2022), we examine whether the effect of local newspaper closures on greenwashing differs by firm size. Prior studies suggest that larger and more visible firms attract greater media attention and are therefore subject to stronger external monitoring (Miller, 2006). Consistent with this view, Heese et al. (2022) show that the impact of newspaper closures is more pronounced for activities associated with more visible firms, as closures weaken external oversight by reducing media coverage. From this perspective, local newspapers play an important role in enabling external stakeholders, including investors, regulators, and local communities, to monitor the behavior of large and highly visible firms. When a local newspaper closes, this monitoring channel becomes less effective, particularly for firms with high public visibility.

Our expectation is grounded in a motivation and opportunity framework. Legitimacy theory suggests that firms seek social approval by aligning their disclosures with prevailing societal expectations. Within this motivation and opportunity framework, because large firms are more visible and face stronger environmental and social expectations, they have stronger incentives to exhibit a higher perceived risk of greenwashing as a way to manage external pressure. At the same time, heightened scrutiny from the media, regulators, nongovernmental organizations (NGOs), and the public constrains such behavior. Local newspaper closures weaken external monitoring and reduce the likelihood that misrepresentation is detected, thereby expanding the scope for greenwashing. This effect is expected to be stronger for large firms, which combine high visibility with stronger incentives for strategic disclosure.

To test this expectation, we classify firms into small and large groups based on whether total assets fall below or above the year median. Columns (1) and (2) of Table 8 show that the treatment effect is statistically insignificant among small firms, whereas it is positive and statistically significant among large firms. The difference between the two estimated treatment effects is statistically significant at the 1% level. This pattern indicates that newspaper closures increase greenwashing to a greater extent among larger firms, consistent with the view that reductions in local media oversight have greater consequences for firms that are more visible to external stakeholders.

Heterogeneity by Social Capital We examine whether the effect of local newspaper closures on greenwashing varies across communities with different levels of social capital. Social capital reflects shared norms of trust, cooperation, and collective engagement (Hoi et al., 2018). In communities where social capital is low, constraints on opportunistic or self-interested behavior are relatively weak, making external sources of information particularly important. Local newspapers play a central role in this regard by providing information, distributing local news, and facilitating public awareness of corporate activities. When a local newspaper shuts down, communities with limited social capital lose a key channel of information provision that complements their community-based scrutinizing mechanism, thereby creating greater scope for firms to exaggerate their environmental performance.

To test this prediction, we classify counties into high- and low-social-capital groups based on the year median of the social capital provided by the Northeast Regional Center for Rural Development.⁴ Columns (3) and (4) of Table 8 report the results. The treatment effect is positive and statistically significant in low-social-capital counties, while it is small and statistically insignificant in high-social-capital counties, with the difference between the two estimates being statistically significant at conventional levels.

⁴County-level social capital indices are sourced from the Northeast Regional Center for Rural Development (NRCRD). These indices are constructed from indicators of civic norms and the density of social networks and are available for 1990, 1997, 2005, 2009, and 2014. Following standard practice, years without observations are filled by linearly interpolating values from surrounding years.

This pattern is consistent with the view that social capital operates through a community-based scrutinizing mechanism that helps deter opportunistic behavior. When this mechanism is weak, the loss of local newspaper information provision has a substantially stronger impact on greenwashing. Overall, these findings indicate that social capital moderates the effectiveness of local information channels, with newspaper closures leading to markedly greater greenwashing in communities where external information provision plays a particularly critical role.

Heterogeneity by Internet Usage Following (Gao et al., 2020), (Kyung and Nam, 2023), and (Jiang and Kong, 2023), we interpret internet usage as a proxy for the availability of alternative online news sources and information channels. Prior research suggests that when local newspapers disappear, residents may increasingly rely on online media for information, raising the possibility that digital platforms partially substitute for the monitoring role traditionally played by local newspapers. Consistent with this substitution perspective, residential internet usage is correlated with online news consumption (Gao et al., 2020) and thus captures individuals’ access to alternative information sources. In the absence of local newspaper coverage, higher internet usage can facilitate outsider information acquisition and oversight of local corporate activities (Kyung and Nam, 2023), thereby reducing the information impact of a newspaper closure. According to Kyung and Nam (2023), when outsiders can more readily obtain information from online channels, insiders are less willing to exploit informational frictions for opportunistic purposes, highlighting the monitoring role of internet-based media.

We test this mechanism using state-year internet usage data from the National Telecommunications and Information Administration (NTIA), which surveys U.S. households’ internet adoption. Counties are classified into high- and low-internet-use groups based on the within-year median of internet usage.⁵ Columns (5) and (6) of Table 8 report the results.

⁵The NTIA reports the percentage of each state’s population that uses the internet. We rely on these state-level aggregates as proxies for county-level internet usage where more granular data are unavailable.

The increase in greenwashing following newspaper closures is positive and statistically significant in low-internet-usage counties, whereas the estimated effect in high-internet-usage counties is small and statistically insignificant.

Importantly, a formal difference test indicates that the difference between the two treatment effects is statistically significant at the 5% level. Taken together, these findings suggest that internet usage operates as an alternative information channel, consistent with Kyung and Nam (2023), whereby online news sources enhance outsider oversight when local newspapers disappear, partially compensating for the loss of local journalistic coverage and limiting firms’ ability to exploit a weakened information environment through greenwashing.

Heterogeneity by Firm Incentives To explore whether firms’ reporting incentives shape their response to newspaper closures, we exploit cross-sectional variation in discretionary accruals (DACC), which proxy for managers’ propensity to engage in earnings management and reflect broader incentives for strategic financial reporting (Jones, 1991; Dechow et al., 1995; Healy and Wahlen, 1999; Kothari et al., 2005). Higher discretionary accruals are commonly interpreted as indicating greater reporting discretion and a stronger willingness to manipulate accounting outcomes, whereas lower accruals suggest more conservative reporting behavior.

Table 9 reports results based on discretionary accruals estimated using the Jones (1991) model and the Kothari et al. (2005) model. For each measure, firms are split into low- and high-DACC groups based on the within-year median. Columns (1) and (2), which rely on the Jones (1991) measure, show that the effect of newspaper closures on greenwashing is positive and statistically significant among firms with high DACC, while the estimated effect is small and statistically insignificant among low-DACC firms. Columns (3) and (4), based on the Kothari et al. (2005) measure, reveal a similar pattern.

Importantly, formal tests indicate that the difference between the treatment effects across

For years without NTIA survey coverage, we follow Gao et al. (2020) and interpolate usage rates between adjacent survey years.

the high- and low-DACC groups is statistically significant at conventional levels. These findings suggest that the increase in greenwashing following newspaper closures is concentrated among firms with stronger reporting incentives and greater discretion over financial disclosures. When local newspapers close and external monitoring weakens, firms that are already more likely to manage reported information appear more likely to extend such strategic behavior to environmental disclosures. Overall, the DACC results reinforce the interpretation that newspaper closures are associated with a greater incidence of greenwashing among firms with a higher capacity and willingness to engage in strategic reporting when oversight declines.

Heterogeneity in Capital Market Effects We next examine whether the capital market consequences of local newspaper closures vary with firms’ pre-treatment greenwashing intensity. Firms differ substantially in the perceived risk of greenwashing, which is captured by the gap between their external environmental claims and internal environmental activities, and this gap may shape how firms respond to changes in the local information environment. Firms with higher pre-treatment greenwashing may benefit more from a decline in external information provision, as reduced local scrutiny lowers the likelihood that symbolic claims are challenged or corrected.

To examine this mechanism, we classify treated firms into low- and high-greenwashing groups based on their greenwashing intensity measured prior to the newspaper closure. Firms that are never exposed to newspaper closures are retained as the control group in both subsamples. Columns (1) and (2) of Table 10 report the results using Tobin’s Q as the dependent variable. The estimated treatment effect is negative and statistically significant for firms with low pre-treatment greenwashing, indicating that these firms experience a relative decline in market valuation following newspaper closures. In contrast, the treatment effect is positive and statistically significant for firms with high pre-treatment greenwashing, suggesting that these firms experience a relative improvement in market valuation when local

newspaper coverage declines.

Importantly, a formal test confirms that the difference between the treatment effects across the two subsamples is statistically significant at the 1% level. This asymmetry suggests that the decline in local information provision does not affect all firms equally. Firms with greater reliance on symbolic environmental disclosure prior to newspaper closures appear to benefit from a weaker information environment. As local journalistic oversight declines, capital markets become less effective at distinguishing substantive environmental performance from greenwashing.

5 Conclusion

This paper studies how the decline of local journalistic infrastructure affects firms' environmental disclosure behavior, focusing on greenwashing. Greenwashing is defined as the gap between firms' external environmental claims and their internal environmental activities, which reflects the perceived risk of greenwashing faced by external stakeholders. Using county-level newspaper closures as plausibly exogenous shocks to local media oversight, this paper examines whether and how reductions in local journalistic coverage influence firms' incentives to engage in greenwashing.

Using a firm-county matched panel of U.S. publicly listed firms from 2002 to 2017, We implement a difference-in-differences research design with firm and state-year fixed effects. The baseline results show that firms headquartered in counties that experience a newspaper closure significantly increase their level of greenwashing. This finding is robust to alternative event windows, placebo tests, treatment definitions, multiple greenwashing measures, the inclusion of county-level controls, and alternative fixed effects structures. Dynamic specifications show no evidence of differential pre-trends, while greenwashing increases only after newspaper closures and continues to rise in subsequent years. A placebo test that assigns newspaper closures to earlier years yields no significant effects, reinforcing the causal inter-

pretation that the observed increase in greenwashing is driven by the loss of local journalistic oversight.

A series of heterogeneity analyses helps clarify the mechanisms behind these results. The evidence consistently indicates that newspaper closures have stronger effects on greenwashing in settings where local newspapers play a more important role in external monitoring and where firms have stronger incentives and greater scope to respond strategically to reduced scrutiny.

Specifically, the increase in greenwashing following newspaper closures is more pronounced among larger firms. Firm size captures corporate visibility and exposure to external monitoring. For highly visible firms, local newspapers serve as an important channel through which investors, regulators, and local communities observe and evaluate corporate behavior. When this channel weakens, the reduction in oversight is larger, making greenwashing more attractive for these firms.

The effect is also concentrated in communities with low social capital. In such communities, informal community-based oversight and social monitoring are weaker, which makes local newspapers particularly important for providing public information and accountability. The closure of a local newspaper therefore creates a larger monitoring gap, allowing firms greater room to exaggerate their environmental performance.

In addition, the effect is weaker in environments where alternative information channels can substitute for local newspapers. In particular, the increase in greenwashing is concentrated in counties with low residential internet usage, while it is much smaller in high-internet areas. This pattern suggests that online media can partially compensate for the loss of local journalism by enabling external stakeholders to obtain information and monitor firms through digital channels.

Firms' internal reporting incentives further shape their response to newspaper closures. The increase in greenwashing is concentrated among firms with higher discretionary accruals, which proxy for greater reporting discretion and stronger incentives for strategic disclosure.

Firms with lower discretionary accruals exhibit little response, indicating that reduced external monitoring mainly affects firms that are already more inclined to manage reported information.

Beyond disclosure behavior, these changes have meaningful capital market consequences. Firms with higher pre-treatment greenwashing experience relatively more favorable valuation effects following newspaper closures, whereas firms with low pre-treatment greenwashing experience declines in market valuation. This asymmetry suggests that when local journalistic oversight weakens, capital markets become less effective at distinguishing substantive environmental performance from greenwashing.

Taken together, the evidence shows that local newspaper closures increase greenwashing primarily by weakening external monitoring in settings where newspapers are most important for oversight and where firms have both strong incentives and sufficient flexibility to adjust their disclosure behavior. The results highlight the central role of local journalism as an informal governance mechanism that disciplines corporate environmental communication.

This paper contributes to the literature by providing new evidence on the governance role of local media in environmental disclosure. It shows that declines in local journalistic coverage weaken external scrutiny in ways that encourage greenwashing and affect market valuation. More broadly, the findings emphasize how firm visibility, community characteristics, digital information access, and managerial incentives jointly shape the consequences of media decline.

From a policy perspective, the results underscore the broader societal costs of the erosion of local journalism. As ESG reporting remains largely voluntary and reputation-driven, the weakening of traditional information intermediaries can create monitoring gaps that undermine the credibility of corporate environmental disclosure. Policies that support local news ecosystems or strengthen alternative community-level information channels may therefore complement formal environmental regulation.

Several limitations remain. Although the research design is rigorous, unobservable time-

varying shocks correlated with newspaper closures cannot be fully ruled out. The greenwashing measures rely on structured data and may not capture all forms of symbolic environmental communication. In addition, the analysis focuses on U.S. public firms, and future research could examine whether similar mechanisms operate in other institutional settings.

Future research may explore how digital platforms and social media interact with traditional local news outlets in shaping corporate environmental behavior. Extending the analysis to other CSR domains, such as labor practices or community engagement, may further improve our understanding of how shocks to local information environments influence corporate conduct.

In summary, this paper shows that local newspaper closures causally increase corporate greenwashing. The findings highlight the importance of local journalism in sustaining external monitoring and promoting environmental transparency, and they point to significant governance consequences associated with the continued decline of traditional media institutions.

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Tables

Table 1: Sample Selection

This table reports the sample construction steps. Column *Observations* shows total firm-year rows, *Firms* the number of unique firms, and *Industries* the number of unique industries based on the first two digits of the Standard Industry Classification (SIC) codes. The initial panel comes from WRDS fundamentals. We then restrict to observations with Asset4 greenwashing ratios, and finally keep those with matched 10-K historical headquarters locations.

	Observations	Firms	Industries
WRDS firm-year observations (2002–2017)	178,380	22,895	73
Asset4 greenwashing ratios available (2002–2017)	18,520	3,153	67
10-K historical HQ location matched (2002–2017)	15,223	2,701	67
Less: Missing control variables and incomplete data (2002–2017)	9,226	1,566	65
Final sample	9,226	1,566	65

Table 2: Yearly Distribution of Treated and Control Firms

This table presents the yearly distribution of firm-year observations by treatment status for the sample period 2002–2017. Column “No Closure” reports the number of firm-years located in counties without a newspaper closure in that year, while column “Closure” reports the number of firm-years located in counties experiencing a newspaper closure. The final column reports the yearly total number of firm-year observations.

Year	No Closure (0)	Closure (1)	Total
2002	106	15	121
2003	118	37	155
2004	171	61	232
2005	221	65	286
2006	245	66	311
2007	287	77	364
2008	389	88	477
2009	475	141	616
2010	496	206	702
2011	497	202	699
2012	485	181	666
2013	481	147	628
2014	475	121	596
2015	747	117	864
2016	1,074	204	1,278
2017	1,022	209	1,231
Total	7,289	1,937	9,226

Table 3: Summary Statistics

This table reports the summary statistics of the variables used in our analyses. All variables are defined in the Appendix A1.

Firm-Years Sample*(N = 9,226)*

Variable	Mean	Std.	P25	Median	P75
Greenwashing	-2.451	0.704	-2.917	-2.483	-2.028
Firm Size	2.187	1.696	1.243	2.285	3.367
ROA	0.042	0.111	0.016	0.052	0.094
Fixed Asset	0.235	0.222	0.064	0.161	0.345
R&D	0.030	0.063	0.000	0.000	0.030
Debt Ratio	0.245	0.197	0.093	0.218	0.351
Capital Expenditure	0.043	0.044	0.014	0.030	0.054
Risk	1.156	1.452	0.383	0.726	1.300
Ln Income per capita	10.935	0.372	10.679	10.875	11.093
Ln County Population	13.807	1.069	13.241	13.809	14.339
%Population Change	0.010	0.085	0.003	0.007	0.014
%Employment	0.774	0.314	0.614	0.680	0.782

Variable	Closure Counties			Non-closure Counties		
	Mean	Median	SD	Mean	Median	SD
Greenwashing	-2.458	-2.484	0.722	-2.449	-2.482	0.670
Firm Size	2.109	2.097	1.635	2.208	2.322	1.711
ROA	0.046	0.051	0.094	0.041	0.052	0.115
Fixed Asset	0.242	0.162	0.229	0.233	0.160	0.220
R&D	0.032	0.001	0.057	0.030	0.000	0.065
Debt Ratio	0.234	0.203	0.197	0.247	0.222	0.197
Capital Expenditure	0.046	0.030	0.051	0.042	0.030	0.042
Risk	1.303	0.848	1.582	1.117	0.705	1.412
Ln Income per capita	10.930	10.894	0.269	10.936	10.856	0.394
Ln County Population	14.021	14.028	0.713	13.750	13.776	1.138
%Population Change	0.012	0.010	0.100	0.009	0.007	0.081
%Employment	0.699	0.665	0.139	0.793	0.695	0.343
<i>N</i>	1,937			7,289		

Table 4: Effect of Newspaper Closures on Greenwashing

This table presents the estimates of the baseline model. The dependent variable is the standardized greenwashing ratio (GW). The key explanatory variable, Treatment, equals 1 for treated firms in the five years following a local newspaper closure (event years $t = 1$ to $t = 5$) and 0 otherwise; the event year itself ($t = 0$) is excluded from the analysis. All firm-level and county-level control variables are included and are defined in Appendix A1. All continuous control variables are winsorized at the 1st and 99th percentiles. The specification includes firm fixed effects and state-year fixed effects. Standard errors are clustered at the state-year level. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% two-tailed levels, respectively.

Stepwise Inclusion of Controls (Firm vs. Firm + County Level)		
Dependent variable: Greenwashing		
VARIABLES	(1) Firm-Level Controls	(2) Firm + County-Level Controls
Treatment	0.104*** (0.034)	0.109*** (0.034)
Firm Size	0.120*** (0.021)	0.114*** (0.021)
ROA	0.052 (0.085)	0.052 (0.086)
Fixed Asset	-0.220* (0.129)	-0.257** (0.129)
R&D	-0.652** (0.301)	-0.638** (0.304)
Debt Ratio	0.030 (0.069)	0.029 (0.070)
Capital Expenditure	-0.406 (0.290)	-0.381 (0.294)
Risk	0.021** (0.009)	0.022** (0.010)
Ln Income per capita		0.304* (0.182)
Ln County Population		0.022 (0.058)
%Population Change		1.831 (1.329)
%Employment		-0.237 (0.179)
Constant	-2.671*** (0.059)	-6.118*** (2.303)
Firm FE	Yes	Yes
State-Year FE	Yes	Yes
Observations	9,226	9,185
Adj. R-squared	0.614	0.615

Table 5: Dynamic Effects of Newspaper Closures on Greenwashing

This table presents the dynamic treatment effects estimated using an event-study specification around the year of local newspaper closure. The event year ($t = 0$) serves as the benchmark category, so the coefficients represent relative changes in greenwashing before and after the closure. All continuous control variables are winsorized at the 1st and 99th percentiles. Firm fixed effects and state-by-year fixed effects are included. Robust standard errors clustered at the state-by-year level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% two-tailed levels, respectively.

Dependent variable: Greenwashing	
VARIABLES	(1) Dynamic Effects
Before_5y	0.089 (0.065)
Before_4y	-0.015 (0.062)
Before_3y	-0.030 (0.044)
Before_2y	-0.021 (0.047)
Before_1y	0.036 (0.047)
After_1y	0.047 (0.036)
After_2y	0.121*** (0.032)
After_3y	0.169*** (0.037)
After_4y	0.186*** (0.042)
After_5y	0.147*** (0.049)
Controls	Yes
Constant	-2.672*** (0.057)
Firm FE	Yes
State-Year FE	Yes
Observations	9,473
Adj. R-squared	0.619

Table 6: Placebo Test: Shifting Closure Year Three Years Earlier

This table reports a placebo test in which we artificially shift the actual newspaper closure year three years earlier and reconstruct the event-time structure accordingly. We re-estimate the baseline model using the same specification as in Table 4. The placebo treatment variable equals one for the five years following the pseudo closure ($t = 1$ to $t = 5$). All continuous control variables are winsorized at the 1st and 99th percentiles. Firm fixed effects and state-by-year fixed effects are included. Robust standard errors clustered at the state-by-year level are reported in parentheses. ***, **, and * indicate statistical significance at the 1%, 5%, and 10% two-tailed levels, respectively.

Dependent variable: Greenwashing	
VARIABLES	(1) Placebo (-3 years)
Treatment_placebo	-0.036 (0.043)
Firm Size	0.103*** (0.020)
ROA	0.088 (0.093)
Fixed Asset	-0.211 (0.140)
R&D	-0.717** (0.291)
Debt Ratio	0.029 (0.076)
Capital Expenditure	-0.168 (0.325)
Risk	0.021* (0.011)
Constant	-2.629*** (0.062)
Firm FE	Yes
State-Year FE	Yes
Observations	8,378
Adj. R-squared	0.610

Table 7: Robustness Tests: Event Windows, Fixed Effects, and Alternative Greenwashing Measures

Panel A reports robustness tests using four alternative event windows around the newspaper closure year. For each specification, the sample is restricted to the corresponding pre- and post-closure intervals. Columns (1) and (2) exclude the event year ($t = 0$), whereas Columns (3) and (4) include $t = 0$ in the post-closure window. Panel B presents results from four regression specifications that differ in their fixed-effects structures. Panel C compares results across three alternative measures of greenwashing. Column (1) uses the benchmark symbolic-minus-substantive measure based on same-year actions. Column (2) measures greenwashing as the difference between symbolic CSR items in year t and substantial CSR items measured in year $t - 1$. Column (3) follows Marcus et al. (2016) and is constructed using the Trucost dataset. All models control for the same firm characteristics. All continuous control variables are winsorized at the 1st and 99th percentiles. Robust standard errors are clustered at the state-by-year level. Panel A and Panel C include firm fixed effects and state-year fixed effects. ***, **, and * denote significance at the 1%, 5%, and 10% levels.

Panel A: Varying the Length of the Event Window				
VARIABLES	(1) [-4, -1], [1, 4]	(2) [-3, -1], [1, 3]	(3) [-5, -1], [0, 4]	(4) [-4, 0], [1, 5]
Dependent variable: Greenwashing				
Treatment	0.109*** (0.035)	0.088** (0.036)	0.058* (0.031)	0.116*** (0.028)
Controls	Yes	Yes	Yes	Yes
Constant	-2.670*** (0.060)	-2.669*** (0.062)	-2.659*** (0.058)	-2.677*** (0.058)
Firm FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Observations	8,943	8,546	9,303	9,360
Adj. R-squared	0.618	0.620	0.706	0.623
Panel B: Alternative Fixed Effects Specifications				
VARIABLES	(1)	(2)	(3)	(4)
Dependent variable: Greenwashing				
Treatment	0.087*** (0.024)	0.104*** (0.034)	0.062** (0.029)	0.130*** (0.037)
Controls	Yes	Yes	Yes	Yes
Constant	-2.760*** (0.059)	-2.671*** (0.059)	-2.616*** (0.065)	-2.615*** (0.066)
Firm FE	Yes	Yes	Yes	Yes
State-Year FE	No	Yes	No	Yes
Industry-Year FE	No	No	Yes	Yes
Observations	9,303	9,226	9,172	9,083
Adj. R-squared	0.588	0.614	0.628	0.642
Panel C: Alternative Measures of Greenwashing				
VARIABLES	(1) GW benchmark	(2) GW one-year lag	(3) GW Trucost	
Dependent variable: Greenwashing				
Treatment	0.104*** (0.034)	0.117*** (0.038)	2.274* (1.361)	
Controls	Yes	Yes	Yes	
Constant	-2.671*** (0.059)	-2.647*** (0.083)	-15.586*** (3.699)	
Firm FE	Yes	Yes	Yes	
State-Year FE	Yes	Yes	Yes	
Observations	9,226	7,477	5,459	
Adj. R-squared	0.614	0.549	0.666	

Table 8: Heterogeneity Tests: Firm Size, Social Capital, and Internet Usage

This table reports heterogeneity tests examining whether the effect of local newspaper closures on corporate greenwashing varies with firm size, social capital, and internet usage. Firms are split by the year median of each characteristic. Columns (1)–(2) present results by firm size, Columns (3)–(4) by social capital, and Columns (5)–(6) by internet usage. The dependent variable is the greenwashing ratio. All continuous control variables are winsorized at the 1st and 99th percentiles. Firm fixed effects and state-by-year fixed effects are included. Robust standard errors clustered at the state-by-year level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% two-tailed levels, respectively.

Dependent variable: Greenwashing						
VARIABLES	Firm Size		Social Capital		Internet Usage	
	Small	Large	Low SC	High SC	Low Internet	High Internet
Treatment	0.082 (0.081)	0.109*** (0.042)	0.141*** (0.049)	0.028 (0.051)	0.203*** (0.061)	0.026 (0.041)
Firm Size	0.114*** (0.042)	0.076*** (0.026)	0.076*** (0.023)	0.113*** (0.040)	0.138*** (0.032)	0.116*** (0.029)
ROA	0.102 (0.092)	-0.018 (0.135)	0.140 (0.108)	-0.118 (0.108)	0.019 (0.139)	0.043 (0.111)
Fixed Asset	-0.440** (0.204)	-0.416* (0.217)	-0.255 (0.166)	-0.441 (0.278)	0.087 (0.203)	-0.507*** (0.168)
R&D	-0.475 (0.332)	-0.163 (0.779)	-0.807*** (0.366)	-0.661* (0.373)	-0.952** (0.392)	-0.551 (0.467)
Debt Ratio	0.034 (0.110)	0.012 (0.111)	-0.021 (0.092)	-0.001 (0.119)	-0.151 (0.102)	0.194** (0.085)
Capital Expenditure	0.100 (0.430)	-0.790* (0.419)	0.031 (0.350)	-0.912*** (0.430)	-0.702* (0.407)	-0.306 (0.429)
Risk	0.015 (0.012)	0.014 (0.022)	0.033*** (0.013)	-0.006 (0.011)	0.016 (0.013)	0.016 (0.013)
Constant	-2.837*** (0.073)	-2.382*** (0.102)	-2.593*** (0.075)	-2.517*** (0.113)	-2.761*** (0.099)	-2.589*** (0.075)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,900	6,075	6,960	2,090	4,876	4,297
Adj. R^2	0.729	0.573	0.599	0.862	0.613	0.634

Table 9: Heterogeneity Tests: Reporting Incentives

This table reports heterogeneity tests based on firms' reporting incentives. Reporting incentives are proxied by discretionary accruals (DACC) estimated using the Jones (1991) and Kothari et al. (2005) models. For each measure, firms are split by the year median into low- and high-value groups. The dependent variable is the greenwashing ratio. All continuous control variables are winsorized at the 1st and 99th percentiles. Firm fixed effects and state-by-year fixed effects are included. Robust standard errors clustered at the state-by-year level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% two-tailed levels, respectively.

Dependent variable: Greenwashing				
VARIABLES	DACC (Jones, 1991)		DACC (Kothari et al., 2005)	
	(1) Low	(2) High	(3) Low	(4) High
Treatment	0.015 (0.078)	0.114*** (0.042)	0.018 (0.075)	0.112*** (0.043)
Firm Size	0.125*** (0.042)	0.086*** (0.026)	0.123*** (0.042)	0.086*** (0.027)
ROA	-0.040 (0.157)	0.065 (0.123)	-0.036 (0.152)	0.022 (0.132)
Fixed Asset	-0.074 (0.270)	-0.273 (0.213)	-0.154 (0.262)	-0.428** (0.207)
R&D	-1.097 (0.901)	-0.469 (0.431)	-1.488 (0.942)	-0.489 (0.453)
Debt Ratio	0.210 (0.146)	-0.007 (0.101)	0.161 (0.144)	0.013 (0.102)
Capital Expenditure	0.262 (0.624)	-0.506 (0.409)	0.553 (0.597)	-0.443 (0.400)
Risk	0.037* (0.022)	0.024 (0.015)	0.033 (0.022)	0.025 (0.015)
Constant	-2.834*** (0.148)	-2.538*** (0.080)	-2.800*** (0.149)	-2.510*** (0.080)
Firm FE	Yes	Yes	Yes	Yes
State-Year FE	Yes	Yes	Yes	Yes
Observations	2,895	5,339	2,967	5,262
Adj. R^2	0.583	0.611	0.591	0.608

Table 10: Heterogeneity by Pre-Treatment Greenwashing

This table reports heterogeneity tests based on firms' pre-treatment greenwashing intensity. Columns (1) and (2) compare firms with low versus high greenwashing prior to local newspaper closures. In both specifications, firms that are never exposed to newspaper closures are included as the control group. The dependent variable is Tobin's Q. All continuous control variables are winsorized at the 1st and 99th percentiles. Firm fixed effects and state-by-year fixed effects are included. Robust standard errors clustered at the state-by-year level are reported in parentheses. ***, **, and * indicate significance at the 1%, 5%, and 10% two-tailed levels, respectively.

Dependent variable: Tobin's Q		
VARIABLES	(1)	(2)
	Low GW	High GW
Treatment	-0.172** (0.082)	0.103* (0.057)
Firm Size	-0.435*** (0.046)	-0.355*** (0.043)
ROA	2.168*** (0.218)	2.043*** (0.219)
Fixed Asset	-1.375*** (0.260)	-0.921*** (0.255)
R&D	6.126*** (0.918)	6.106*** (1.015)
Debt Ratio	0.079 (0.162)	0.021 (0.158)
Capital Expenditure	5.385*** (0.674)	4.797*** (0.677)
Risk	0.057** (0.026)	0.046* (0.026)
Constant	2.927*** (0.142)	2.678*** (0.138)
Firm FE	Yes	Yes
State-Year FE	Yes	Yes
Observations	8,084	8,317
Adj. R-squared	0.796	0.804

Figures

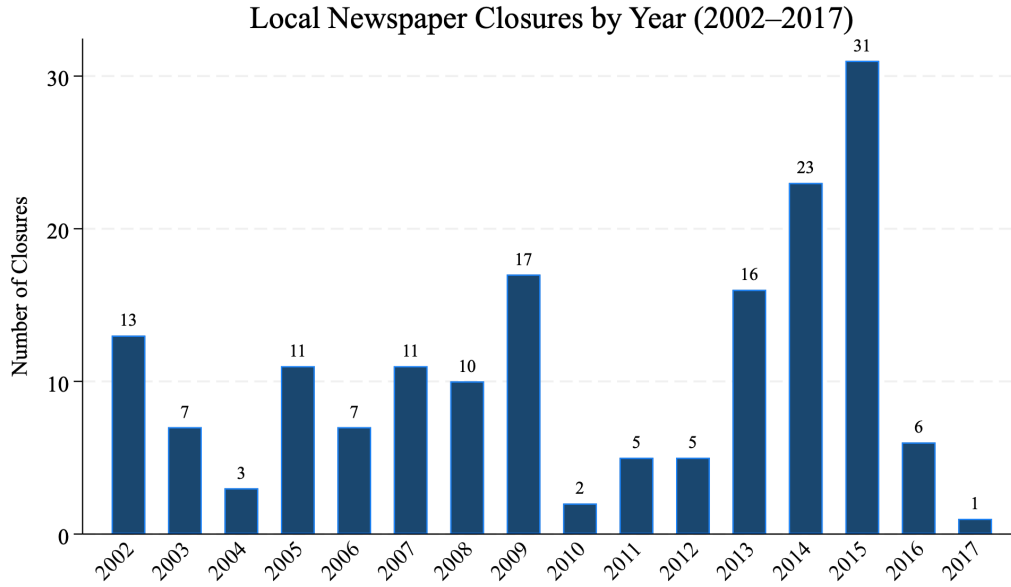


Figure 1: Local Newspaper Closures by Year, 2002–2017

Figure 1 reports the annual number of local newspaper closures from 2002 to 2017. The closure data are sourced from Jiang and Kong (2023), who compile newspaper cessation dates using two primary references: *Chronicling America*, an electronic archive maintained by the Library of Congress, and the *Editor and Publisher Yearbook*, an annual directory of U.S. newspapers. Jiang and Kong (2023) retain only the first newspaper closure in each county with facilities in the EPA’s TRI database. The figure summarizes the distribution of these verified closure events across the years covered by our sample.

Appendix A1. Variable Definitions

Variable Name	Definition	Source
Dependent Variable		
Greenwashing Ratio	The difference between symbolic and substantial CSR items, both measured at year t .	Refinitiv ESG; Hawn and Ioannou (2016)
Variable of Interest		
Treatment	A binary variable that equals 1 for firm-year observations in counties that experienced a local newspaper closure and are observed in the post-closure window (event years $t = +1$ to $t = +5$). It equals 0 for all pre-closure observations (event years $t = -5$ to $t = -1$) and for all firms in counties without a closure.	Jiang and Kong (2023); <i>Chronicle America</i> ; Editor & Publisher Yearbook
Firm-Level Characteristics		
Firm Size	Natural logarithm of total employees (emp).	Compustat
ROA	Return on assets, calculated as net income (ni) divided by total assets (at).	Compustat
Fixed Asset	Capital intensity, calculated as net property, plant, and equipment (ppent) divided by total assets (at).	Compustat
R&D	Research and development expense (xrd) divided by total assets (at).	Compustat
Debt Ratio	Total debt (dt) divided by total assets (at).	Compustat
Capital Expenditures	Capital expenditures (capx) divided by total assets (at).	Compustat
Risk	Inverse of solvency ratio, calculated as stockholders equity (teq) divided by total liabilities (lt).	Compustat
County-Level Characteristics		
Ln Income Per Capita	The natural logarithm of the income per capita for each county year.	BEA (CAINC1)
Ln County Population	The natural logarithm of county-year population.	BEA; U.S. Census Bureau
% Population Change	Percentage change of county population relative to the previous year.	BEA; U.S. Census Bureau
% Employment	County annual employment deflated by population in each year.	BEA (CAINC4)

Appendix A2:

Substantive and Symbolic CSR Indices' Items (Hawn & Ioannou, 2016; Bothello et al., 2023)

Panel A: Substantive CSR Items

Item No.	Asset4 Code	Definition
1	CGBFDP019	Percentage of non-executive board members on the audit committee as stipulated by the company.
2	CGBFDP023	Percentage of non-executive board members on the nomination committee.
3	CGBFO03S	Does the company have an audit committee with at least three members and at least one financial expert?
4	CGBSD01S	Does the company have a policy for maintaining a well-balanced membership of the board?
5	CGBSO07S	Percentage of independent board members as reported by the company.
6	CGBSO17V	Percentage of women on the board of directors.
7	CGCPD01S	Does the company have a policy for performance-oriented compensation to attract and retain senior executives and board members?
8	CGCPO05S	Do the company's statutes or by-laws require stock options to be granted only with shareholder approval?
9	CGSRD01S	Does the company have a policy ensuring equal treatment of minority shareholders or facilitating shareholder engagement?
10	CGVSDP005	Does the company have a CSR committee or dedicated CSR team?
11	ENERDP0011	Does the company have a policy to reduce emissions?
12	ENPIO08S	Does the company develop products or technologies for water treatment, purification, or improved water-use efficiency?
13	ENRRDP0011	Does the company have a policy to improve water efficiency?
14	ENRRDP0012	Does the company have a policy to improve energy efficiency?
15	ENRRDP046	Does the company make use of renewable energy?
16	ENRRDP058	Does the company use environmental criteria (e.g., ISO 14000, energy consumption) in supplier selection?
17	SODODP0012	Does the company have a work-life balance policy and a diversity and equal opportunity policy?
18	SOEQD01S	Does the company have a competitive employee benefits policy and ensure good employee relations in its supply chain?
19	SOHRD01S	Does the company have a policy guaranteeing freedom of association and excluding child or forced labor?
20	SOHSD01S	Does the company have a policy to improve employee health and safety within the company and its supply chain?
21	SOTDD01S	Does the company have a policy supporting employee skills training or career development?

Panel B: Symbolic CSR Items

Item No.	Asset4 Code	Definition
1	CGVSDP016	Does the company openly report challenges or trade-offs in integrating financial and non-financial issues?
2	CGVSDP028	Is the company's CSR report published in accordance with GRI guidelines?
3	CGVSDP029	Does the company's extra-financial report cover its global activities?
4	CGVSDP030	Does the company employ an external auditor for its CSR or sustainability report?
5	ENERDP031	Does the company report initiatives to recycle, reduce, or substitute ozone-depleting substances?
6	ENERDP036	Does the company report initiatives to reduce or phase out volatile organic compounds (VOC)?
7	ENERDP081	Does the company report initiatives to reduce the environmental impact of transportation?
8	ENERO05S	Does the company report initiatives to reduce or compensate CO ₂ emissions in production?
9	ENERO08S	Does the company report initiatives to reduce or phase out SO _x or NO _x emissions?
10	ENERO14S	Does the company report initiatives to recycle or reduce total waste?
11	ENRRDP052	Does the company report environmentally friendly or green sites or offices?
12	ENRRO03S	Does the company report initiatives to reduce or phase out toxic chemicals?
13	SOCOD01V	Does the company report policies on business ethics, corporate citizenship, or endorsement of global principles?
14	SOCODP053	Does the company report crisis management or reputation recovery systems?
15	SODODP026	Does the company claim to provide flexible working hours supporting work-life balance?
16	SODODP027	Does the company claim to provide daycare services for employees?
17	SOEQDP0201	Does the company claim to provide bonus plans to most employees?
18	SOEQDP025	Does the company claim to provide pension funds, healthcare, or insurance to employees?
19	SOHRDP026	Does the company report using human rights criteria in supplier selection or monitoring?
20	SOHRDP029	Does the company report readiness to end partnerships if human rights criteria are violated?
21	SOHSDP039	Does the company report policies or programs addressing HIV/AIDS in the workplace?
22	SOPRDP029	Does the company report developing products with specific health or safety benefits for consumers?
23	SOTDDP023	Does the company claim to favor promotion from within?
24	SOTDDP024	Does the company claim to provide regular training for managers?

Appendix A3: List of County-Level Newspaper Closures

This appendix table reports the list of county-level daily newspaper closures used in the analysis. The closure information is obtained from Jiang and Kong (2023), who compile a comprehensive list of U.S. newspaper cessations by combining *Chronicling America* and the *Editor & Publisher Yearbook*. The entries in this appendix correspond to those reported in Appendix Table A1 of Jiang and Kong (2023). For each county, the Combined FIPS code is constructed using the official county–FIPS mapping published by the U.S. Census Bureau, which we merge with the newspaper-closure dataset to obtain standardized state and county identifiers.

State	County	Closure Year	Combined FIPS
Alabama	Colbert	2013	1033
Alabama	Jefferson	2005	1073
Alabama	Madison	2015	1089
Alabama	Marengo	2009	1091
Alabama	Mobile	2014	1097
Alaska	Anchorage Municipality	2014	2020
Arizona	Pima	2009	4019
Arizona	Yuma	2001	4027
Arkansas	Craighead	2005	5031
Arkansas	Saline	2011	5125
California	Alameda	2002	6001
California	Contra Costa	2006	6013
California	Kern	2006	6029
California	Placer	2015	6061
California	San Benito	2008	6069
California	San Bernardino	2015	6071
California	San Francisco	2016	6075
California	San Joaquin	2007	6077
California	Santa Clara	2008	6085
California	Santa Cruz	2000	6087
California	Stanislaus	2004	6099
California	Ventura	2002	6111
Colorado	Arapahoe	2014	8005
Colorado	Denver	2009	8031
Colorado	Routt	2015	8107
Connecticut	Fairfield	2015	9001
Connecticut	Hartford	2009	9003
Connecticut	New London	2011	9011
District of Columbia	District of Columbia	2014	11001
Florida	Broward	2000	12011

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State	County	Closure Year	Combined FIPS
Florida	Hernando	2014	12053
Florida	Hillsborough	2016	12057
Florida	Indian River	2002	12061
Florida	Lee	2009	12071
Florida	Martin	2002	12085
Florida	Miami-Dade	2015	12086
Florida	Palm Beach	2013	12099
Florida	Pinellas	2011	12103
Florida	Volusia	2001	12127
Georgia	Chatham	2008	13051
Georgia	Clarke	2001	13059
Georgia	Coweta	2016	13077
Georgia	Forsyth	2014	13117
Georgia	Fulton	2001	13121
Georgia	Glynn	2016	13127
Georgia	Henry	2005	13151
Georgia	Houston	2003	13153
Georgia	Muscogee	2006	13215
Hawaii	Honolulu	2010	15003
Idaho	Canyon	2018	16027
Idaho	Cassia	2008	16031
Idaho	Shoshone	2013	16079
Illinois	Clay	2007	17025
Illinois	Coles	2010	17029
Illinois	Ford	2007	17053
Illinois	Kane	2016	17089
Illinois	Lake	2006	17097
Illinois	St. Clair	2000	17163
Illinois	Will	2014	17197
Illinois	Winnebago	2015	17201
Indiana	Allen	2015	18003
Indiana	Clark	2011	18019
Indiana	Elkhart	2001	18039
Indiana	Floyd	2011	18043
Indiana	LaPorte	2014	18091
Indiana	Rush	2015	18139
Indiana	Wabash	2002	18169
Iowa	Cherokee	2002	19035
Iowa	Emmet	2015	19063
Kansas	Atchison	2006	20005
Kansas	Butler	2015	20015

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State	County	Closure Year	Combined FIPS
Kansas	Cherokee	2014	20021
Kansas	Cowley	2016	20035
Kansas	Johnson	2008	20091
Kansas	Montgomery	2014	20125
Kansas	Norton	2002	20137
Kansas	Rice	2005	20159
Kansas	Riley	2012	20161
Kansas	Sedgwick	2009	20173
Kansas	Seward	2008	20175
Kansas	Sherman	2008	20181
Kentucky	Franklin	2015	21073
Kentucky	Kenton	2007	21117
Louisiana	Beauregard Parish	2015	22011
Louisiana	Calcasieu Parish	2015	22019
Louisiana	St. Tammany Parish	2005	22103
Maryland	Cecil	2015	24015
Maryland	Dorchester	2008	24019
Maryland	Frederick	2002	24021
Maryland	Montgomery	2005	24031
Maryland	Prince George's	2006	24033
Maryland	Washington	2007	24043
Massachusetts	Essex	2000	25009
Massachusetts	Hampden	2003	25013
Massachusetts	Suffolk	2009	25025
Michigan	Bay	2009	26017
Michigan	Berrien	2015	26021
Michigan	Calhoun	2001	26025
Michigan	Genesee	2009	26049
Michigan	Livingston	2000	26093
Michigan	Macomb	2014	26099
Michigan	Oakland	2012	26125
Michigan	Saginaw	2009	26145
Michigan	Wayne	2009	26163
Minnesota	Goodhue	2014	27049
Minnesota	Nobles	2017	27105
Minnesota	Washington	2015	27163
Mississippi	Coahoma	2015	28027
Mississippi	DeSoto	2009	28033
Mississippi	Grenada	2009	28043
Mississippi	Jones	2014	28067
Missouri	Jackson	2012	29095

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State	County	Closure Year	Combined FIPS
Missouri	Macon	2015	29121
Nebraska	Otoe	2006	31131
Nevada	Clark	2005	32003
Nevada	Humboldt	2008	32013
Nevada	Washoe	2014	32031
New Hampshire	Grafton	2012	33009
New Jersey	Cumberland	2014	34011
New Jersey	Salem	2015	34033
New Mexico	Lea	2005	35025
New York	Onondaga	2001	36067
New York	Orleans	2013	36073
North Carolina	Lenoir	2015	37107
North Carolina	New Hanover	2003	37129
North Carolina	Rockingham	2009	37157
North Carolina	Union	2014	37179
Ohio	Butler	2013	39017
Ohio	Crawford	2013	39033
Ohio	Cuyahoga	2015	39035
Ohio	Greene	2007	39057
Ohio	Hamilton	2007	39061
Oklahoma	Garvin	2014	40049
Oklahoma	Hughes	2007	40063
Oklahoma	Kay	2007	40071
Oklahoma	Mayes	2014	40097
Oklahoma	Oklahoma	2003	40109
Oklahoma	Okmulgee	2002	40111
Oklahoma	Payne	2007	40119
Oregon	Coos	2013	41011
Oregon	Union	2014	41061
Pennsylvania	Allegheny	2015	42003
Pennsylvania	Berks	2002	42011
Pennsylvania	Chester	2013	42029
Pennsylvania	Clinton	2015	42035
Pennsylvania	Erie	2000	42049
Pennsylvania	Lackawanna	2005	42069
Pennsylvania	Lancaster	2009	42071
Pennsylvania	Lycoming	2015	42081
Pennsylvania	Mercer	2013	42085
Pennsylvania	Montgomery	2014	42091
Pennsylvania	Montour	2015	42093
Pennsylvania	Northumberland	2003	42097

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State	County	Closure Year	Combined FIPS
Pennsylvania	Union	2003	42119
Pennsylvania	Venango	2015	42121
Pennsylvania	Westmoreland	2000	42129
Rhode Island	Washington	2013	44009
South Carolina	Oconee	2009	45073
Tennessee	Cocke	2015	47029
Texas	Burnet	2015	48053
Texas	Collin	2013	48085
Texas	Deaf Smith	2014	48117
Texas	Galveston	2004	48167
Texas	Gregg	2014	48183
Texas	Harris	2013	48201
Texas	Hidalgo	2005	48215
Texas	Kaufman	2013	48257
Texas	Limestone	2009	48293
Texas	Matagorda	2002	48321
Texas	Orange	2013	48361
Texas	Potter	2001	48375
Texas	Williamson	2013	48491
Utah	Salt Lake	2003	49035
Virginia	Fairfax	2001	51059
Virginia	Alexandria city	2001	51510
Virginia	Hopewell city	2007	51670
Washington	Grays Harbor	2013	53027
Washington	Lewis	2014	53041
Washington	Snohomish	2008	53061
West Virginia	Harrison	2002	54033
West Virginia	Kanawha	2015	54039
West Virginia	McDowell	2002	54047
Wisconsin	Brown	2005	55009
Wisconsin	Rock	2012	55105
Wisconsin	Wood	2015	55141
Wyoming	Albany	2004	56001